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## **Model and Metaphor in Social Anthropology**

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It is a truism that as social scientists we are constrained in our thinking by the dominant beliefs and intellectual climate of our time. Kaplan (1964) has pointed out that some values in scientific beliefs arise through what he calls key metaphors in various periods of history: fruitful comparisons which stimulate the imagination of the age. The eighteenth century was given to clockwork conceptions, the nineteenth to organismic ideas; and now in the twentieth century we tend to make scientific formulations in terms of the workings of a computer.

What is noticeable about these conceptual devices is that (a) semantically they are all metaphors, (b) functionally they all serve to relate events or discoveries into coherent systems, (c) formally they are in some sense models of reality for heuristic purposes, and (d) they all involve the implied use of analogy.

The intention in this paper is to review the use of model and metaphor in one social science, social anthropology; to come in this context to a better understanding of the working distinction between these terms; and finally to appraise their usefulness and comprehensiveness for this discipline.

### **EARLY MODELS**

The model in some sense has been at least implicitly used since the beginning of social anthropology. It was originally connected with social change, a subject in which many workers of the nineteenth and early twentieth centuries were interested. Against a background of Darwinian

evolutionism and contemporary anthropological and historical studies (e.g. Bastian 1860; Buckle 1857-61; Lubbock 1855; Waitz 1858) they assumed that a given society was both stable and at a certain stage of development. The problem was then to discover or surmise what had happened to it before this stage, and what further changes it was likely to experience in the future.

In general terms, all societies were posited as passing in social evolution through the stages of savagery and barbarism to civilization. This was an organismic model into which various societies examined (usually from the literature) could be fitted and classified. The apotheosis of this scheme was of course Victorian society from which the investigators themselves sprang and had their being. Nevertheless, it had all the rough-and-ready taxonomic advantages of an early model. Moreover, as a good model surely should, it stimulated research. Using this model, Morgan (1878) and McLennan (1897) devised an evolutionary sequence for the family. Corresponding sequences for law were suggested by Maine (1897) in England and Letourneau (1890) in France. In the sphere of religion, Tylor (1865) and Frazer (1890) postulated appropriate stages, as did the French workers Lefevre (1891) and Mauss (1896).

Many of these writers accepted foreign and bizarre customs as evidence for the validity of postulated but unobserved stages of the model. Where fact would not support fiction, the model was deduced by argument. Paternity, it was maintained, could never have arisen until the stage of polygamy was reached. Hence patrilineal de-

scent and inheritance necessarily belonged to the higher forms of society (Achelis 1890).

Models of this kind, with only one central theme and few detailed ramifications, might be called *unithematic*. They seem to have an essential place in the early stages of enquiry when the field needs to be delineated in broad black and white terms. As more sophisticated material comes in, they become more and more naive and unsatisfactory, and, their job done, are finally repudiated.

As the evolutionary unithematic model was gradually abandoned in the face of mounting scientific criticism, its place was taken for a while by a diffusion model. The problem here was to trace the spread of culture items, mainly techniques and material objects, through past migrations. Apparently, however, the time was not yet ripe for inductive verification by fieldwork, although the model made this possible and desirable. Perry (1927) tried very persistently to show from the literature that wherever practices such as building pyramids, mummifying the dead and wearing gold and pearl ornaments existed, they had been introduced by the ancient Egyptians, or by people in contact with them. This model must still be described as unithematic.

Hogbin (1958), to whom much of the material for this summary is due, believes that in England a modern reorientation set in just before 1920. The younger anthropologists realised that the method of conjectural history, as Radcliffe-Brown later called it (1952), was unscientific because the conclusions could never be verified. At the same time it came to be generally accepted that students should not only visit the areas of investigation themselves, but should learn the vernacular and engage in participant observation. Yet the influence of the earlier evolutionary model and its operators was felt for another decade, at least to the extent that students continued to be attracted by the unfamiliar. They selected remote societies for study, and where they were obliged to investigate a culture which had "broken down", they tried through the tribal elders to recapture the unspoilt past. Malinowski himself, to whom so much of modern fieldwork technique is owed, makes only casual references to the Europeans and missionaries who had been living in the Trobriand Islands for fully twenty years before he first reached them in 1915 (Malinowski 1922; 1935).

Still another characteristic was involved in the

new model to come. As Mitchell has indicated (1961), Malinowski, and Radcliffe-Brown in his earlier period (1922), studied small-scale island communities in which it was possible to see the whole society in action. Redfield denoted this as the "period of study of simple societies conceived as self-contained autonomous societies" (1955). Whether such a limitation of field helped to determine the *functional biological* model or was in turn determined by it is difficult to say at this distance. In any event, the new model transfixed in immobility societies which had previously been seen in process of change. The model required that the parts of society should be seen in functional interdependence, after the analogy of the need-satisfying components of a biological organism. It was hard enough to analyse out the social parts in interrelationship without the added difficulty of treating these parts as variables in social change. The institutions of society were accordingly taken as constants in terms of the needs they met. This connected with a further presupposition—important for later work—that their total interactional condition was one of stable equilibrium. It is significant that Malinowski's later attempts on the basis of this model to interpret social change and "culture contact" are generally acknowledged to be the least successful of his work (Gluckman 1948; Malinowski 1945; Mitchell 1961). On the other hand, it was obvious from his writings (e.g. 1926), that Malinowski was fully emancipated from the errors of "conjecture or hypothetical reconstruction" and was anxious that his model of society should correspond with reality. This is the first anthropological model which could be called truly *polythematic*.

#### **SOCIAL STRUCTURE MODELS**

It fell mainly to Radcliffe-Brown, influenced no doubt by his philosophical and logical training at Cambridge, to develop the functional biological model into a *biological structural* one. Functional interdependence on the analogy between social and organic life was retained (1952) and consciously referred back to its early formulation in Durkheim (1919). While the dangers of the biological analogy were fully realised, it was extended in a not particularly helpful way to designate areas of study as social morphology, social physiology and social evolution (Radcliffe-Brown 1952). The concept of general human needs taken up by Malinowski was played down by Radcliffe-Brown,

and replaced by a narrower and more technical definition in terms of "necessary conditions of existence".

This was required because of the notion of "structure" which Radcliffe-Brown superimposed on the biological model. Even in Malinowski's usage, the social organism was a closed structure or system, in the sense that it had outer physical limits and was internally self-functioning. Moreover, the term "structure" was already in current use, often in reference to the form of persistent social groups. But it was Radcliffe-Brown who in social anthropology gave ordered expression to the concept and embodied it in his functional working hypothesis. Using a metaphor, he defined social structure as a "network of actually existing social relations", but included also the differentiation of individuals and classes by social role. This structural emphasis led attention away from the organismic aspects of the model, which in effect became metaphorical in character. Human needs were less directly important than the harmonious functioning together of the parts of the social structure. Function was now defined in terms of "sufficient . . . internal consistency, i.e. without producing conflicts which can neither be resolved nor regulated" (1952: p. 181).

While the assumption of general human needs could be put aside in this scheme, Malinowski's concept of stable equilibrium was as important as ever. In terms of the organic analogy, Radcliffe-Brown considered deviations from stability as pathological. Disequilibrium was given a value judgment as a diseased condition of the social organism, producing *dysnomia*, disorder, social ill-health, as opposed to the *eunomia* of the healthy integrated society (1952: p. 182). This value assumption was to have unfortunate consequences, not only for the effective study of social change, but for later investigations of large-scale, complex societies.

Homans was an early critic of the biological structural model. He found fault with the argument that because some recurrent activity is "organically" interrelated with other activities, it necessarily makes a contribution to social survival. "The interrelatedness of the elements of social behaviour may be dysfunctional as well as functional", he wrote (Homans 1951: p. 271). Hogbin, too, has questioned Radcliffe-Brown's organic reification of society: "There is no point in saying

that a certain custom has an integrative function for the society as a whole; and to state that society in a state of *dysnomia* always struggles towards *eunomia* is to cloud the issues. It is rather individual persons who combine into new groups when they find that some common aim is best achieved thereby . . ." (Hogbin 1958: p. 29).

The idea is tempting that over the years the biological aspect of the biological structural model has dwindled from analogy through metaphor into meaninglessness. What is clear is that (a) once the tendency to functional interdependence of at least clusters of social institutions was accepted, the organismic analogy had fulfilled its purpose, and (b) the social structure model is capable of separate logically self-consistent existence. The result has been that various forms of social structure models have made their appearance without overt biological implications. Even these, however, have rested on the presuppositions of closure and stability characteristic of their biological prototypes.

Leach has criticized these presuppositions, and in the process has produced another type of structural model (1954). Anthropologists, he says, are almost alone in regarding social change as shattering and somehow fundamentally immoral. Their prejudice in favour of integration, functional consistency and structural equilibrium is the outcome of the conditions under which they work. An analysis torn out of time and space in the course of a year or two's fieldwork requires the axiom of equilibrium, for without it the model would appear to be incomplete. But they go too far in assuming that the equilibrium is stable, i.e. firmly established and unlikely to suffer sudden change.

When the anthropologist attempts to describe a social system, Leach goes on, he necessarily describes only a model of the social reality. The model represents the anthropologist's hypothesis of "how the social system works", and is therefore a coherent whole. This does not imply, however, that social reality is a coherent whole: on the contrary it is full of inconsistencies, which provide an understanding of social change. But in practical fieldwork situations the material observed must be treated as *if* it were part of an equilibrium system, or description would be "almost impossible". All that Leach asks is that the fictitious nature of this equilibrium be fully recognised. His description of a social system provides an *idealized*

model which states the "correct" status relations existing between groups within the total social system, and between the social persons who make up the particular groups.

The logical attempts made here to reconcile the model with reality are interesting, and may be summarized under the heading of *dissociation*. In contrast with Radcliffe-Brown, the model is firstly of idealized status relations as opposed to actual "person-to-person" ones. It is an *idealized structural model*, which allows unwanted idiosyncratic variations to be dissociated or excluded. Again, the equilibrium of the model is dissociated from the disequilibrium of reality, and the two are connected only by a tenuous "as-if" relationship. The model marks an advance in (a) dispensing with the biological analogy which had become only a metaphor, (b) recognizing fully the non-integrated nature of more complex societies, and (c) opening the door to an analysis of social change. The price paid is the new problem of the criteria by which the anthropologist's model, since it is dissociated from reality, shall be tested against it.

Anthropologists are not alone in being castigated for regarding social systems as closed and social change as shattering. Dahrendorf (1958) has taken the sociologists to task for much the same fault. He has, he says, yet to see a problem for the explanation of which the assumption of a unified value system is necessary, or a testable prediction which follows from this assumption. One of the more unfortunate connotations of the word "system" is its closure. Although some structural functionalists have tried to, there is no getting away from the fact that a system—if only for "purpose of analysis"—is self-sufficient, internally consistent, and closed to the outside. By no feat of the imagination can the integrated and equilibrated social system be made to produce serious and patterned conflicts in its structure. What it does produce is the well-known villain of the piece, the deviant. He quotes early Parsons: "Deviance is a motivated tendency for an actor to behave in contravention of one or more institutionalized normative patterns" (1951; p. 250); and deviance occurs "either if an individual happens to be pathological, or if, from whatever source, a disturbance is introduced into the system" (p. 252). Dahrendorf recommends as a corrective a problem-conscious discipline (1958: p. 124).

Problems require explanation; explanations require assumptions or models, and hypotheses derived from such models. These hypotheses are always, by implication, predictions as well as explanatory propositions, and require testing by further facts. Testing in turn often generates new problems. He also emphasizes the usefulness of a *conflict model* of society, based on the presuppositions that (a) continuous social change is taking place unless some force intervenes to arrest it, (b) social conflict is ubiquitous: its absence is surprising and abnormal, and (c) societies are held together not by consensus but by constraint. Dahrendorf does not insist that the conflict model is the only one. Problems for study can be selected in terms of the equilibrium model or the conflict one, or perhaps of other models too. He points out, though, that the models with which we work, apart from being useful tools, determine to no small extent our general perspectives, our selection of problems and the emphasis of our explanations. He might also have indicated that these factors help to determine our models.

In social anthropology, Gluckman and his co-workers, generalizing initially on rural fieldwork in Central Africa, have for long urged that conflict, ambiguity and inconsistency are characteristic of social change. Gluckman has emphasized that conflicts in men's allegiances in one set of relationships lead to cohesion through cross-cutting alliances over a wider range of relations, or through a longer period of time (Gluckman 1955). Mitchell has crystallized the concept of conflict in the notion of the "plural society" (1961), after Furnivall (1948). Here, disparate systems of customs and beliefs coexist, and are called into action in different social situations. Mitchell does not entirely accept Gluckman's theory of "counterbalancing cleavages", but seems more to agree with Dahrendorf that constraint rather than consensus is the basis of cohesion in plural societies. His eventual working model of "complex reticulations of social relationships" does not, however, appear to differ greatly from Epstein's "different sets of social relations or spheres of social interaction" (1958: p. xvii). Gluckman has expressed a similar view of "loose, semi-independent, to some extent isolated sub-systems" (1961: p. 80). With all these scholars, despite their full recognition of conflict and inconsistency, it is not difficult to discern the underlying search for a

systematic model: the "cross-cutting alliances" of Gluckman, the "complex reticulations" of Mitchell, and Epstein's "spheres of social interaction". It would appear that the social structure model has been forced into "semi-independent sub-systems" mainly because of the disconnected nature of the data themselves.

There are other versions of the social structure model which by their formulation avoid, or at least accept more naturally, the pressures imposed by social disunity. One of the most valuable is that of Nadel (1957), who, following Parsons (1954), defines the structure of a society as the pattern or network (or "system") of relationships between "actors in their capacity of playing roles relative to one another" (Nadel 1957: p. 12). The notion of role has been curiously neglected as a formal concept in anthropological models (Reader 1961: p. 212). It enables Nadel to make the important distinction between corporate groups, in which membership has all the characteristics of role-performance, and institutionalized social relationships, analysis of which proceeds *pari passu* (1957: p. 60). Occasionally, says Nadel, anthropologists have chosen a mode of presentation whereby they single out particular roles and outline social structure on this basis, reaching it by way of a role inventory (e.g. certain kinship roles in Eggan 1950; Fortes 1949). Generally, however, roles are only described as they become relevant.

The great advantage of the role structure model is that it provides effortlessly for conflict, ambiguity and inconsistency without further presuppositions such as "as-if" relations or plural societies. Nobody has any difficulty in visualizing incompatible roles performed by one and the same individual or group; roles which are ambiguous, either deliberately in a fluid social situation or because they are new and have not had time to crystallize; or roles which are inconsistent in one social situation compared with another. Moreover, Nadel makes full allowance for cleavages in the structure. Absence of a common logical locus, he says, precludes the assumption of a unitary, coherent role system. Indeed, there seem to be as many separate systems as there are logical role-frames. Between them there is only the linkage provided by recruitment rules, defining the flow or "circulation" of persons between disparate sets of roles, and the chances of their belonging to several at once (Nadel 1957: p. 97).

Nadel is also able to deal with the principle of equilibrium. Purely objectively, and ignoring the viewpoint of welfare, he says, human communities in a state of equilibrium are neither better nor worse, neither more nor less interesting, than any other state. The assumption of equilibrium is important only in that it makes sense to look for determinacy; and the constancies observed in field anthropology are certainly of short range (1957: p. 145). It is clear, however, that the short-term constancy of a particular logical role frame, not changing its shape "as soon as our backs are turned", is all that is required to satisfy the conditions of Nadel's model. If no workable constancies emerge, then there is no society to study and no structure to define.

Borrowing from Lévi-Strauss (1953: p. 528) Nadel calls social structure a "statistical model", in the sense that it has the same degree of reality (or, as he says, non-reality) which would be ascribed to any purely statistical picture of a social situation. By contrast there are "mechanical models" of societies, exemplified by their valid laws: marriage laws, for example "calling for actual groupings of the individuals according to clan and kin". It is only the pragmatic design of societies, their body of rules backed by sanctions, which can be ascribed concrete efficacy and "real" consequences. The statistical model can have no such effectiveness, but provides only "thresholds" (Lévi-Strauss), zones of indeterminacy, and hence indices of the probability which its constancies apply (Nadel 1957: pp. 147-148).

This attempt to operate an idealized structural form through the medium of an *actual* structural model resting on the tradition of Malinowski and Radcliffe-Brown, leads to severe difficulties. Radcliffe-Brown believed he knew what he wanted as the basis of social structure—the "concrete reality" of actually existing social relations (1952: p. 192). Gluckman, in an intermediate position, builds his model on "typical" or representative events (1942: p. 245). These may not accurately describe any actual social events, but represent the *type* of behaviour that underlies actual events in a given community. This formulation shows a keener appreciation of the inductive first-degree-abstraction process by which field data are classified from the level of observation. Finally, the possibility arises, as utilized by Leach, of

stating the "correct" or idealized relations which the anthropologist thinks should exist between social persons.

At this point it may be useful to make a distinction between *ideal*, *idealized* and *ideational* models. An *ideal* model is built on what are sometimes called normative patterns of behaviour. These are at the level of descriptions by informants, or comments by them, on what "ideal" in the sense of perfect or rational behaviour should be. In the courts of law such behaviour is often laid down, and actual behaviour assessed against it, in terms of the concept defined by Gluckman in the "reasonable (and upright) man" (1955b: p. 22). In an idealized model, on the other hand, "the concepts and relations chosen . . . are not given by nature, but are largely the invention of the investigator. They are governed primarily by the way in which he thinks it 'profitable to represent experience'" (Firth 1954: p. 7). Such, according to Firth, is the economist's model. Lévi-Strauss, too, points out that "social structure has nothing to do with empirical reality but with models built after it"; and these must be such that "they make immediately intelligible all the observed facts" (1953: p. 525). Leach, similarly, seems to think that structural analysis is only tenuously connected with reality. The structures which the anthropologist describes, he says, are nothing more real than "models which exist as logical constructions in his mind." (Leach 1954: pp. 5, 9).

Nadel makes an attempt to square the *idealized* models of Leach and Lévi-Strauss with his own inclinations, moulded by the British school of anthropology, towards an *empirical model*. All empirical models, by definition, are inductive: that is to say, their terms and relations are arrived at by the logical process of generalization from repeated, similar, particular instances of observed phenomena. This abstraction, at the first level, is performed in the very act of recognizing and separating out a particular phenomenon for description (Beattie 1959: p. 48). We thus never analyse reality directly, and to think that we do is to misunderstand fundamentally the process of induction. The "concrete reality of social structure" is a logical contradiction: the basic terms of any model, social structural or otherwise, are at least first-level abstractions. Empirical models are therefore at least inductive, second-level generalizations of first-level abstractions derived from

observation. So are ideal models, but they are based on normative conceptions as framed by informants. Ideational models, as will presently emerge, may or may not be inductive. Idealized ones are by definition not inductive but deductive. They are convenient, imaginative constructions, deduced from general experience to fit "reality" as seen by the operator.

Nadel seems both to have misconceived induction and to have been torn between empirical and idealized models. Following Radcliffe-Brown (1952: p. 192), he writes: "I consider social structure, of whatever degree of refinement, to be still the social reality itself, or an aspect of it, not the logic behind it; and I consider structural analysis to be no more than a descriptive method, however sophisticated, not a piece of explanation." (Nadel 1957: pp. 150-151). He admits, after Braithwaite (1953: pp. 90-91, 108) that a model implies more than this, namely a "picture" so constructed that it has a logical necessity and explanatory power, so that verifiable deductions can be made from it. But he does not think that "social structure" satisfies this more rigorous condition. In point of fact, if the present analysis is correct, Nadel could have found satisfaction in either of two broad types of model: the empirical one, generalized from reality and modelled on it, or the idealized one, deduced from general social experience and capable of being verified by reference to it. Instead, his position forced him to the regrettable conclusion that what makes structural analysis informative is not the final picture at all, but the analytic steps that lead up to it (1957: p. 154).

To complete this analysis, there remains to be considered the *ideational* model—the model of social ideas. A classical exposition of this type is in the social philosophy of Emile Durkheim (trans. 1953). Sociology, he says, studies a normative model of society, and not the distorted model constructed from an expression of public opinion (1953: p. xvi). There is an enormous gap between the way in which values are actually estimated by the ordinary individual and the objective scale of human values which should in principle govern our judgments. The average moral conscience is mediocre; it feels only slightly the commonest duties, and is blind to some (1953: p. 83). Moreover, because a certain condition is found in a large number of people, it is not for that reason "objective". The "general" phenomenon itself, in

relation to the behaviour of the majority of individuals, may be of relatively infrequent occurrence (p. xii). Such a conception of social fact rests on the assumption that society is a system of ideas, beliefs and sentiments, working through, but over and above, the individual minds and ideas falling within it (p. 59).

This ideational model deals best, as one might expect, with the ideological and value aspects of society. Civilization, Durkheim points out, is the result of the co-operation of men in association through successive generations. It is essentially a social product: a "congregation" of the highest human values. We can receive from this storehouse of intellectual and moral riches at most a few fragments. The more we advance in time, the more complex and immense does our civilization become. Consequently, the more does it transcend the individual consciousness, and the smaller does the individual feel in relation to it (Durkheim 1953: p. 54). Again, Durkheim contends that Socrates expressed more clearly than his judges the morality suited to his time. It would be easy to show that as a result of the transformation of the old society based on the gens, and the consequent disturbance of religious beliefs, a new morality and religious faith had become necessary in Athens. This was not felt by Socrates alone, but also in a powerful current of opinion represented by the Sophists. It was in this sense that Socrates was ahead of his time, presaging social change, while at the same time expressing the spirit of the age (1953: pp. 64-65). Durkheim's analysis of "moments of collective ferment", at which are born the great ideals upon which civilizations rest, is also illuminating (pp. 91-92).

On the negative side, severe distortions and assumptions have to be made to accommodate individuals and their relationships to the ideational model. In order to fit the scheme, induced behaviours have to be converted to ideas in the form of "social facts", which are then reified. When, for example, one examines not individual suicides, but the rate of suicide, "this total is not simply a sum of independent units, a collective total, but is itself a new fact *sui generis*, with . . . its own nature . . . dominantly social" (Durkheim 1953: p. 46). This approach did not, however, prevent Durkheim from approaching the problem of suicide with a statistical treatment much in advance of his time.

Even ideas themselves cannot be subsumed

under the model at the individual level, for "society" is something over and above them. Collective representations, produced by action and reaction between individual minds, are accordingly created, on a fallacious analogy with the supposed emergence of individual representations (ideas) from the interaction of neural elements in the brain (1953: p. 27). Individual minds are not, so far as we know, like neural elements. Neither is the connexion between individual minds and collective representations (if these exist) at all demonstrably like that between neural elements and individual representations. The "emergence" of collective representations itself is a metaphor, again highly dubious, based on chemical combination, in which the product has properties over and above its interactive elements.

Such conceptual devices are required to fulfil at least three functions in Durkheim's scheme: (1) to maintain all elements of the model at the same ideational logical level, so that it can be structured; (2) to explain the normative force of society admittedly felt by its members; (3) to separate sociology from individual psychology. Thus Durkheim, in his model, is forced to reify the social facts and collective representations of society. He holds that social facts should be approached as if they had a reality independent of the observer. Collective representations provide a conceptual framework of action (1953: p. xxii). Society as thus constituted provides "an established classification" of values, outside individuals, "which is not their own work, which expresses other than their own personal sentiments, and to which they are bound to conform" (p. 84). Although Durkheim elsewhere makes it clear that individual choice is possible within and even against the conceptual framework of collective representations (e.g. p. 61), a constant effort of will is needed so as not to confuse the emergent structure of society with "a monstrous Group Mind" (p. xxiii).

Functionalism as previously considered is not necessary to Durkheim's model, which is accordingly freed from the social immobility inherent in functional models. As Peristiany points out, "Durkheimian society does not balance 'as in a budget', ends achieved with energy spent. Its standard of value is not that of the happiness of the greater number or of the average citizen. It is not social utility or even the survival of society under its material form." (Durkheim 1953: p. xxviii).



Durkheim himself declares that to explain the function of ideas by the contribution they make to the maintenance of the equilibrium, the solidarity or the survival of a society is to misconstrue the central tenet of his sociology, that individuals are subordinated to society as a system of ideas

## MODEL THEORY

The survey of structural and pre-structural model-types which has just been made may broadly be summarized in a table. This does not presume to show the chronological or other influence of any one form of model upon another:

	DEDUCTIVE	DEDUCTIVE/ INDUCTIVE	INDUCTIVE (Empirical)
UNITHEMATIC	Evolutionary (1855-1897) Diffusion (e.g. Perry 1927)	Diffusion (e.g. Rivers 1914)	
POLYTHEMATIC		Ideational (Durkheim 1919) Functional-Biological (Malinowski 1922-1945) Biological-Structural (Radcliffe-Brown 1939-1952) Idealized (Leach 1954)	Conflict (Gluckman, Mitchell, Epstein 1942-1961) Ideal (Gluckman 1955) Role-Structure (Nadel 1957)

N.B.—The above dates are only a rough guide in terms of significant publications.

(p. xxviii). This both explains the persistence of customs, as when individuals change their social environment but cling to their ideals as symbols of social identity, and leaves the way open for social change in terms of new ideas.

It must finally be observed that ideational models can logically be either inductive or deductive in form. Durkheim offers a composite model in which behaviour may first be induced in terms of statistical rates. The result, however, becomes a unit on a new *sui generis* supra-individual level, and is connected deductively with other units on the strength of generalizations derived from analogy and metaphor. Moreover, the relationship between "social facts" as thus produced and "collective representations", whose mode of inference is not clear, is not fully worked out. A simpler ideational model might well be evolved deductively, as when one postulates from general experience what are the operative ideas of a society; or inductively, as when they are inferred from questioning representative samples of social populations.

It may be proposed, however, that increasing knowledge of society has so far tended to bring about a change from deductive to inductive models with time. At the beginning, in the absence of empirical knowledge, an idealized scheme has to be superimposed on the limited data available, which their sparsity is often unable to contradict. As empirical knowledge grows, inductive procedures tend to shape the model more and more, for otherwise it will not cover, and is not verified by, field data. The second and more sophisticated return to deduction in social science is suggested by Leach (and by Firth and the economists). The idealized deductive model is not in fact "profitable" as a representation of reality until the operator already possesses a profound inductive knowledge by experience of the society he is investigating. It is, as Firth says, "a deliberate construct, simplified from, or departing from, real life situations for heuristic purposes" (1954: p. 6). It has been described in the more developed science of economics as "a closed symbolic representation of the interaction of certain econo-

mic phenomena" (Stone and Jackson 1946: p. 555).

From the various considerations adduced in the present survey of models, it would appear that a social model in any form may serve the following purposes:

1. To make description and visualization of reality, or of real problems, more plain;
2. To provide general interrelation or explanation of as wide a range of facts as possible in terms of the minimal number of inter-connected general propositions or symbols of them, resting on the simplest, fewest, and most fully acceptable axioms;
3. To indicate fruitful possibilities for further theorizing, problem-finding and experimentation within the field covered by the model: in a word, to promote further hypotheses;
4. To predict, as in applied anthropology, what will be the outcome of certain combinations of circumstances or variables which are comprised by the model.

These purposes evidently do not differ from those of social theory at large. But the contention is that models are theory making use of a particular logical form: analogy.

#### ANALOGY

It seems logically necessary that all models bearing any reference to reality should be based on analogy. This is indeed a type of inference bearing a strong formal resemblance to induction itself. The induction used in social science is nearly always of the kind called by logicians "induction by simple enumeration" (Stebbing 1930: pp. 246-249). It involves counting a number of instances recognized as having certain properties in common, and proceeding by inference to the assumption that *all* instances have these properties in common. This is to be contrasted on the one hand with "perfect" induction (Aristotle) in which every instance is known, and with "intuitive" induction on the other, the immediate apprehension of an inductive generalization on the strength of one observed instance (Stebbing 1930: pp. 243-244).

Analogy, likewise, involves inference from resemblances. But whereas with simple enumeration the inference is from similarity of the known to the unknown within the same total class of phenomena, in analogy it is between different classes which are alleged or believed to resemble one another in the properties and relationships at

issue. To convert allegation into justified belief, and thus demonstrate analogy, it is necessary to show what Keynes calls a strong "positive analogy" and a weak "negative analogy" (1921) between the properties of members of the original and of the allegedly similar class.\*In other words, the respects in which the properties and relationships of the two classes resemble one another should greatly outweigh the respects in which they differ, *these similarities and differences should be explicitly stated*, and the differences should be irrelevant to the comparison. Since knowledge of the properties and relationships of the classes is never complete, the total analogy will always include properties and relationships which are not known to belong, or not to belong, to the two classes being compared (Stebbing 1930: p. 250).

The suggestion now is that models are a *constructed class*, or a *set of interrelated constructed classes*, with which the properties and relationships of corresponding classes of phenomena seen "in real life" are being compared by analogy. The constructed class may be indicated by "expressive" signs, i.e. words, or by "substitute" signs, i.e. symbols (Stout in Stebbing 1930: p. 115). Examples in the latter case are the deductive symbolic models of the economists and applied mathematicians. These are symbolic representations of the expected relationships between classes of phenomena: that is, synthetic general statements by analogy of what the relationships between corresponding phenomena in real life can be expected to be. They may be grossly simplified or abstracted from reality, and they may be problem-oriented to a degree that involves distortion of reality as a whole. But unless they bear some analogic resemblance to reality, they are surely not models of reality at all; and yet they cannot be "models" of anything else. Without some correspondence with reality, they are useless for that visualization, explanation and prediction of reality†which is their *raison d'être*. Such models may be termed *symbolic* models.

Before proceeding to models expressed in words, the useful *representational* model, not discussed in terms of symbolism by Stout, arises. This is probably closest in conception to the

\*Other considerations, such as "comprehensiveness" (Keynes), are left out for simplicity of presentation.

†For philosophers who hold an "ideal" theory of perception, it may be acceptable that "reality" and the "constructed class" are mainly different sets of sense-data between which a relationship of correspondence holds.

original use of the term "model": it is the physical model with which we are usually first acquainted. In a typical model of the brain, for example, such as is used for anatomical demonstration, the cerebrum in relation to the cerebellum, the mesencephalon, the pons and the medulla oblongata are shown by physical analogy between model and original. The model is a medial form *representing* many original dissected brains. The positive analogy is strong: similarity of form and interrelationships, perhaps colour, a one-to-one correspondence of detail in the original to detail in the model—*arbor vitae* to *arbor vitae*, *brachia pontis* to *brachia pontis*. The negative analogy is unimportant, that is, it does not introduce misleading distortion for demonstration purposes. Plaster of Paris (or whatever material it may be) is not the same as brain tissue; the brain model may not be of the same size as an original brain, and so forth. Even three-dimensionality, however, is not an essential part of the positive analogy, for sufficient representation can be obtained from a two-dimensional brain-atlas with overlapping pictures of successive brain-levels (e.g. Krieg 1957). This might be called a *pictorial* representational model.

That the symbolic, representational and verbal forms of model are all intimately related is well known in an advanced science like physics. "It has often happened in physics", say Einstein and Infeld, "that an essential advance was achieved by carrying out a consistent analogy between two apparently unrelated phenomena" (1961: p. 270). The development of wave mechanics, begun by de Broglie and Schrödinger in 1925-26 is an example of the achievement of a successful theory by means of a deep and fortunate analogy. Similarity was proposed, and verified, in the unlikely analogy between the properties of a standing wave in an oscillating chord (e.g. a violin string) and those of an atom emitting radiation. This "acoustical analogy" (Einstein and Infeld 1961: p. 276) has become, with the special theory of relativity, the foundation of modern wave mechanics. In its working out, recourse must be had to mathematical (symbolic) models and sub-models, but its outline is also simply illustrated with the pictorial and verbal models which occur in the work quoted. Einstein and Infeld carefully point out that an understanding not only of the similarities but of

the differences of the analogy (i.e. the negative analogy) is important (p. 286).

These illustrations bring out what seem to be a number of important points. First, it is clear that the "constructed classes" of a model may be symbolically, verbally or even pictorially constructed independently in the mind of the investigator, or they may be inductions from another, even unlikely, class of phenomena found in nature, with which the class of phenomena under investigation is to be compared by analogy. Second, an important function of the positive analogy is evidently that its known dimensions in the model are to be projected onto corresponding but unknown dimensions in the phenomena under investigation, so that new properties for experimental verification may be suggested about them. The known negative analogy is a stricture of the properties in respect of which the two classes in analogy *cannot* be compared. Again, the "model" is shown as a special form of *theory*, i.e. a synthetic general statement (or connected series of statements) about what the relations between certain recurrent factors can be expected to be (Reader 1961: p. 211), *but a theory stated by analogy*. Finally, it appears that models of all kinds may fulfil several functions: simplified representation (e.g. pictorial models), simplified interconnexion (e.g. symbolic models), simplified projection of properties (e.g. verbal models).

#### METAPHOR

Verbal models, with which in anthropology we are mainly concerned, may be only metaphorical or fully analogic in character. In the earlier anthropological models, and more generally in the absence of inductive information, the place of analogy tended to be taken by metaphor. Perhaps because the concept is so well known, the definition of metaphor has not been easy. The Oxford English Dictionary defines it as "the figure of speech in which a name or descriptive term is transferred to some object different from, but analogous to, that to which it is properly applicable". Aristotle ascribes the use of metaphor to delight in learning; Cicero traces it to the enjoyment of the author's ingenuity in overpassing the immediate, or in the vivid presentation of the principal subject (Cope 1867). These views make metaphor a decoration, an entertainment and a diversion as opposed to its logical use in the

dictionary definition. Ogden and Richards approach it from both the logical and the emotive standpoint: "Metaphor, in the most general sense, is the use of one reference to a group of things between which a given relation holds, for the purpose of facilitating the discrimination of an analogous relation in another group" (1930: p. 213); and again, "(Indirect means of arousal are possible) through the excitement of imagery (often effected at low levels of refinement by the use of metaphor)—used not, as in strict symbolizing, to bring out or stress a structural feature in a reference, but rather to provide, often under cover of a pretence of this elucidation, new sudden and striking collocations of references for the sake of compound effects of contrast, conflict, harmony . . . or used more simply to modify and adjust emotional tone . . ." (p. 240).

The philosopher Black, in a useful article (1954-55), has discussed three viewpoints on metaphor. The first, which he calls the *substitution* view, holds that a metaphorical expression is used in place of some equivalent literal statement. If it is asked why this substitution should be made, one answer, apart from the entertainment or emotive view, is that metaphor is a kind of catachresis. This means the use of a word in some new sense in order to remedy a gap in the vocabulary. If the metaphor thus serves a genuine need, the new sense it introduces should quickly become part of the literal language. Hence, on this view new metaphors should at least sometimes be in

process of being absorbed (Black 1954-55: pp. 279-280).

To hold that a metaphor consists in the presentation of an underlying analogy, or similarity, is to take what Black calls the *comparison* view. When Schopenhauer called a geometrical proof a mousetrap, he was, according to this view, implicitly saying that like a mousetrap, a geometrical proof offers a delusive reward, entices its victim by degrees, leads to a disagreeable surprise, and so on (Black: p. 283). It will be observed that the comparison view is a special case of the substitution view: for it holds that the metaphorical statement might be replaced by an equivalent literal *comparison*.

The view which Black himself favours is the *interaction* view. In Richards' words (1936: p. 93): "...when we use a metaphor we have two thoughts of different things active together and supported by a single word, or phrase, whose meaning is a resultant of their interaction". These "thoughts", according to Black, arise out of the system of *associated commonplaces*: the statements which the reader, or listener, would make as a layman, without taking special thought, of those things which he held to be true about the class invoked by the metaphor. In this form, Black commits the interaction view to the following seven claims. For brevity, they are set out in tabular form, with the present writer's criticisms in terms of the "comparison" view alongside:

#### "INTERACTION" VIEW OF METAPHOR

1. A metaphorical statement has two distinct subjects—a "principal" and a "subsidiary" one.
2. These subjects are often best regarded as "systems of things" rather than "things".
3. The metaphor works by applying to the principal subject a system of "associated implications" characteristic of the associated subject.
4. These implications usually consist of commonplaces about the subsidiary subject, but may, in suitable cases, consist of deviant implications established *ad hoc* by the user.

#### CRITICISM —"COMPARISON" VIEW—

What are these but members of two separate and allegedly parallel classes as compared in analogy?

The "system of things" are surely the sets of properties defining the classes in analogy.

The "associated implications" are surely properties alleged to be held in common—i.e. the positive analogy—between the classes being compared.

In scientific metaphor they may have been carefully thought out—as a positive analogy.

#### **"INTERACTION" VIEW OF METAPHOR—Cont.**

5. The metaphor selects, emphasizes, suppresses, and organizes features of the principal subject by implying statements about it which normally apply to the subsidiary subject.
6. This involves shifts in the meaning of words belonging to the same family or system as the metaphorical expression; and some of these shifts, though not all, may be metaphorical transfer. The subordinate metaphors are, however, to be read less emphatically.
7. There is in general no simple "ground" for the necessary shifts of meaning—no blanket reason why some metaphors work and others fail.

#### **CRITICISM—"COMPARISON" VIEW—Cont.**

i.e. selective positive analogy.

This leads to an infinite regress of sub-metaphors within the meaning of the main metaphor, against which Black barely defends himself.

The sole ground is that the classes drawn in analogy are appropriately or not appropriately compared: i.e. that their positive analogy is or is not strong.

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Black believes that it is easy to overstate the conflicts between these three views, and that they may all be true in different cases. But, he says, only the "reaction" view is of importance in philosophy. The present contention is that only a developed form of the "comparison" viewpoint—what might be called the "*analogic*" view—is useful in social science.

Setting aside its emotive and decorative uses, metaphor would appear to be a summary form of analogy, but unexpanded: that is, with the positive and negative analogies either uninvestigated, unexpressed, or not under control. Metaphor seems an implied analogy in which one, or at most very few, striking items of positive analogy are seized upon, without regard for the rest of the positive, and the probably substantial negative analogy. Conversely, logically unsatisfactory analogies, with disproportionately large negative analogies, are often uninvestigated metaphors expanded into analogies by argument.

Some of these points are well brought out by the comments of Firth on Radcliffe-Brown's use of the "network" metaphor: "Now no one thought that he meant that he was dealing with either a meshed fabric held together by knots, or an arrangement with intersecting lines and interstices recalling such a fabric. We can take it for granted that like a modern painter, when he wrote *network* he was expressing what he felt by describing metaphoric-ally what he saw." (1954: p. 4). Firth is here bringing

to light part of the substantial negative analogy of the class-term "network", which *unfits* it for analogy with the arrangement of relationships between persons. He implies that the metaphor was never meant to be expanded into an analogy, but is only of assistance in "seeing" a non-material situation in material terms. He goes on, "Bentham (1931) has a pertinent remark about the danger of metaphors being used at first for illustration or ornament and afterwards made the basis of an argument . . ." (1954: p. 5).

Since the negative analogy in metaphors, when revealed, is likely to be uncontrolled and high, the metaphor is not, without caution, suitable for use in models. It cannot be maintained, since analogy is involved, that the use of metaphor as or in a model is logically invalid, but only that it is restricted and possibly misleading. Suitably chosen, it could make description and visualization of reality more plain, provide some explanation of a range of interconnected facts, and even be associated with problem-orientation and prediction. It is suggested, however, that it would perform the last two functions much better either when expanded into an analogy or when its implicit analogy is known to be positive and dominant. Otherwise it functions as a model only in assisting the operator to visualize that which is not visible, with some danger from the contra-indications of its unknown negative analogy.

In practice, many of the anthropological models

reviewed above consist of partially expanded metaphors or incomplete analogies: these amount to the same thing. This is particularly true of models based on analogy with the biological organism. To begin with, the positive analogy of this model was artificially strengthened by selecting for the "reality" side of the analogy only small-scale, relatively homogeneous communities. These were obviously more closely analogous to an organism in equilibrium than large-scale urban groups would have been. Even so, the positive analogy was limited and the negative analogy only partially expanded. In the positive analogy were used the properties and relations of functional interdependence of parts and need-satisfaction, both resting on the presuppositions of closure and stable equilibrium. But the unexpanded negative analogy was formidable. The organic parts of a body were not like social persons in interrelationship. Their dispositions and functions (e.g. the functions of the kidney and liver) were entirely different from those of a society. The surface of the body was not like the often arbitrary boundaries of a community, the metabolic cycle and physiological changes had no precise counterpart in society, to speak of the sex of communities was meaningless, and so on indefinitely. Once the fact of "functional" interrelationship in homogeneous societies was grasped and utilized, it is hardly surprising that little further use was found for this analogy and the model it represented. It remains, if at all, as a metaphor, with the few items of relevant positive analogy and the unexpanded negative analogy characteristic of all metaphors.

This analysis of the logic of metaphor has perhaps gone too far in playing down the initial exploratory value of metaphors and their function in stimulating new hypotheses. It is no doubt a natural sequence of thought to proceed from the known to the unknown: to comprehend in one metaphorical leap a similarity between what has already been conceived and that which has still to be understood. The findings of this review suggest, however, that to adhere literally to the implied comparisons of a metaphor in the developmental phase, when the broad association has already been grasped, may well inhibit further insight. If, through a large and unsuitable negative analogy, the metaphor does not bear expansion into a model, it should surely either be discarded when its early heuristic purpose has been achieved,

or its limitations should severely be borne in mind while no better model is available.

In practice the matter is not as clear-cut as this. Metaphors in social anthropology seem to be capable of varying degrees of partial expansion. Indeed there should be a continuum between the suggestive but totally unexpandable metaphor and the one which is so fruitful that it is virtually an analogic model. Somewhere along this continuum comes, for example, the "social network", ably initiated by Barnes (1954) and developed by Bott (1957) and later by Mayer (1961) and others. The "network" is "the total of ego's interpersonal relations with other individuals." (Mayer 1961: p. 9). In Barnes' words: "Each person is . . . in touch with a number of people, some of whom are directly in touch with each other and some of whom are not . . . I find it convenient to talk of a social field of this kind as a *network*. The image I have is of a net of points, some of which are joined by lines. The points of the image are people, or sometimes groups, and the lines indicate which people interact with each other." (Barnes 1954: p. 43). This metaphor has been useful in explaining close-knit and loose-knit\* community relations in Norway, England, and Black South Africa. It is doubtful, though, whether the taxonomy of networks recommended by Mayer (1962) would serve any useful purpose. That is taking the implied analogy too far.

#### SUMMARY

The material of this review can finally be summarized as a set of considerations with respect to social models:

1. The logical form of the model should be clearly understood: whether inductive (empirical) or deductive; whether ideal, idealized or ideational; whether unithematic or polythematic; and whether verbal, symbolic or pictorial. Models should not be set up at mixed logical levels without due caution.
2. Deductive idealized models seem appropriate to the early stages of a discipline, to open research in a little-known area. They also apply in the developed stages when much is already known inductively about the phenomena under review. For the intermediate stages of research, empirical inductive models seem more fitting.

\*Another metaphor. When metaphors are compounded in an implied analogy, its comprehensiveness for a given situation may only be that of the weaker metaphor.

3. Any model, however far "removed from reality" should always permit of inductive verification.
4. A model should interrelate or "explain" the widest possible range of data in terms of the minimal number of connected propositions, based on the simplest, fewest and most fully acceptable presuppositions.
5. It should be problem-oriented, and should promote further hypotheses, or lead to predictions.
6. The positive and negative analogies of the model should be expanded and made manifest as fully as possible, so that the logical validity of the model may be tested.
7. A metaphor should not without caution be used in or as a model; and if it is used:
  - 7.1 it should be expanded into an analogy by making manifest its positive and negative implied analogies; or if not so expanded,
  - 7.2 its purpose in providing a tangible concept for intangible reality should be clearly stated, and its use restricted accordingly.
8. Presuppositions of closure and stability in

social models are arbitrary. Some models should also be based upon, and able to meet, assumptions of:

- 8.1 continuous social change unless disturbed;
- 8.2 ubiquitous conflict, ambiguity and inconsistency;
- 8.3 integration by constraint rather than consensus.

Finally, this review should not be misconstrued as recommending the exclusive and pervasive adoption of model-making as the only major theoretical tool in social science. Kaplan (1964: pp. 277-287) has adequately listed for us the shortcomings of models: their over-emphasis on symbols and on form, their oversimplification, their frequently misplaced rigour without deductive fertility, their misuse as "maps" of pictorial realism, and the danger of the unconscious transition from "that's what it is like" to "that's what it is". However, the dangers, as Kaplan says (p. 292), are not in working with models but in working with too few which are too much alike; and above all in belittling attempts to work with anything else.

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